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Biogeosciences Discussions

Interactive comment on "NW European shelf under climate warming: implications for open ocean – shelf exchange, primary production, and carbon absorption" *by* M. Gröger et al.

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Authors

We thank the referee for the intense review of the manuscript and gratefully appreciate the comments and suggestions. We are confident this results in a substantial improvement of the manuscript.

Referee

The authors present results from a GCM-ESM whose setup and grid-resolution allows regional studies on the NW European shelf. The aim of the study was to identify

changes in ecosystem productivity and potential causes under climate change conditions and the quantification of the potential influence of the shelf sea region to open ocean carbon storage. The study is an interesting contribution to the modelling on the NWE shelf and potentially provides an alternative approach to regional ocean models that requires the prescription of boundary conditions. Nonetheless, I have two major points concerning the representation of the model that need to be thoroughly addressed before publication. First, the validation of the model is inadequate for both the global and the regional performance, but especially for the latter. This needs to be conducted and discussed thoroughly. Second, the manuscript lacks generally discussion of the results, the model performance and model uncertainties. There is a general need to assign references to the processes discussed in case those cannot be derived from the figures presented. The model results need to be discussed with respect to the available literature on SoA modelling of the NWE shelf including especially studies with regional models. Additionally model uncertainties need to be addresses. Some of those were already mentioned in the comment posted by Jason Holt. Others concern the use of only one atmospheric model and one specific scenario. Thus all results drawn from the future scenarios need to be discussed with respective reservations. What is the potential of the model to perform multi-model/multi-scenario ensembles? What are the computational requirements? It seems to be drawback that the full global model has to be spun-up over several thousand years before making the actual simulations. I think these aspects have to be included to provide a comprehensive picture of the method. Although the authors could not convince me that the model performs better on the NWE shelf than a regional North Sea model, I am certain that the model is a useful tool to study specifically processes related to the in/outflow at the shelf break and to link larger scale circulation to the regional scale. Thus, I consider this to be an important contribution to the regional modelling on the NWE shelf that should be published after the two major points and some minor comments (see Supplement) has been sufficiently addressed.

Authors

We see these two major points and are aware that we present a rather novel approach compared to studies that use regional models. Therefore, a profound validation is necessary to assess the model's performance and to get an impression on the uncertainties for the predicted climate change for the 21 century. Hence, we will introduce our modeling approach and discuss the drawbacks and benefits associated with our model compared to the current established models. We agree there is need for a broader discussion of the model's results based on existing literature. Especially the prediction of increasing stratification along the continental slope during the 21st century. Meanwhile we have checked that the stratification, and the weakening vertical mixing is a persistent feature of all cmip5 global simulations carried out with the ECHAM6/MPIOM coupled model. We would like to briefly describe this in the following.

We here compare the historical experiments for the 20th century with the Representative Concentration Pathways (RCP) warming scenarios 4.5 and 8.5. All model runs were conducted using the state of the art coupled atmosphere-ocean GCM ECHAM6/MPIOM/HAMOCC. The scenarios were carried out with two spatial resolutions and in ensembles of respectively three realizations each of them initialized from three different restart files of the preindustrial control run (see Jungclaus, J.H., Fischer, N., Haak, H., Lohmann, K., Marotzke, J., Matei, D., Mikolajewicz, U., Notz, D., von Storch, J.-S. (submitted). Characteristics of the ocean simulations in MPIOM, the ocean component of the MPI Earth System Model, 2013 for details).

In all these scenarios we see a substantial shallowing of the ocean's mixed layer in the Atlantic Ocean north of 40 °N (Fig. 1). Although none of these model setups resolves the North Sea satisfyingly, we note there is a strong shallowing of the mixed layer around the north of Scotland especially along the Scotland-Faroer channel from which waters enter the North Sea (fig. 1). Hence, from these ensemble simulations we can conclude that the mixed layer shallowing appears to be a robust feature throughout the state of the art warming scenarios. Moreover, we note that the mixed layer shallowing is stronger in the high emission scenario RCP 8.5 compared to the moderate scenario

4.5. It is very likely that the mixed layer shallowing in these simulations is also associated with a stronger stratification as this is the case in this study. Additional evidence for a likely shallowing of the North Atlantic mixed layer depth at the end of the 21st century is derived from a multi model study following the SRES A2 scenario (Steinacher et al., 2010, Biogeosciences, 7, 979-1005).). Indeed, all four state of the art coupled atmosphere-ocean-ecosystem models applied in this study showed the characteristic increase in Atlantic stratification together with a profound decline in productivity. From this we conclude that the stratification feedback is not a phenomenon specific to our model but appears to be rather a characteristic feature of warming scenarios seen in many ocean GCMs. We will make this more clear in a revised version.

Specific minor comments:

Referee

Introduction

P16627L23–âĂŘP16628L6 these processes are indeed characteristic for the NWE shelf, but not all of them are relevant for the shelf break. Please be more specific here. Please also make clear what processes can be resolved by your method and how sub–âĂŘgrid scale processes are treated in the model (maybe in the method section).

Authors

We will adapt this paragraph and mention only the relevant processes. (see also our reply to Jason Holt)

Referee

Model description

Since, the carbon cycle is one of the main aims of the study I think a few more details on the carbon module would be interesting.

Authors

We will include a reference to Ilyina et al. (accepted; available online at http://onlinelibrary.wiley.com/doi/10.1002/jame.20017/abstract). The authors provide a very profound description of the model's organic and inorganic carbon cycle as well as the included nitrogen and sulphur cycle along with a substantial validation of global climatologies.

referee

Experiments

River loads: Please include maybe a graph on river loads that indicate the magnitude of the change and the control level. Page 16630 L24–âĂŘ25 : are those 5yr averages or is a 5yr period repeated? Experiment MARKER: what exactly has been done here? Please add scenario description here.

Authors

We will include a graph that shows the river concentrations. After 2006 a 5 year period of monthly means is repeated. A description of ensemble experiment Marker is given in section 6.2. But we will move it to section 3 (experiments).

Referee

Model Validation (see also comment above)

Global Ocean:

The whole paragraph does not only describe the model–âĂŘdata comparison, but also gives a number of explanations that can't be derived from the figures referring e.g. to different water types. Please include references and make clear what conclusions are drawn from your model.

Authors

We will improve this section and provide references to papers validating the global carbon cycle model HAMOCC.

Referee

Fig. 2: Obviously the model doesn't perform equally well for all model regions, which I guess can't be expected from a global model. Nonetheless, this requires a thorough discussion.

– it would be better to plot the difference between data and model (or a relative difference) to identify the performance within specific areas since it is difficult to see this by visual comparison only (same for Fig. 3).

Authors

We think it is a good idea to plot also the differences to the observations in addition to the absolute values. The latter is important to show that the model reproduces adequately the global seasonal nutrient cycle and should therefore provided as well. This is important since seasonal differences are large in mid and high northern latitudes.

The relative differences are clearly impacted by the extreme deviation in the subtropical gyres (although absolute deviation are lowest there) where nutrient concentrations are close to zero. Thus, at first order, relative deviations follow the pattern exhibited by the absolute inventories. This makes an interpretation fairly difficult. In revised version, however, we will comment on relative difference where appropriate (i.e. European shelf).

Referee

What about nitrate?

Authors

As the large scale global nitrate pattern is mainly controlled by the same processes as for phosphate in HAMOCC (nutrient uptake during photosynthesis follows strictly

the Redfield ratio) an additional plot for nitrate gives no further insight to the model's performance on the global scale, since nitrogen specific processes such as N2 fixation and denitrification are tied to rather local environments.

Referee

–âĂŘis there a data gap at 0deg ?

Authors

It's an artifact of the plotting tool. Will be fixed in a revised version.

Referee

P 16632 L14 Antarctic Bottom Water

Authors

will be fixed in the next version

Referee

North Sea (see comment above) The following points need clarification: –âĂŘPlease include an introduction and details about the datasets used.

Authors

We will provide an introduction about the data sets used for the validation in the next version.

Referee

Primary production: I don't think that atmospheric wet deposition would account for more then half of the production. What other reasons are possible?

Authors

Compared to other models HAMOCCs benthic remineralization of organic matter rates

is rather low. As a consequence, the recycling of nutrients is probably lower than in other models which in turn leads to lower gross production. We will make this clear in the next version.

Referee

Fig. 4: –âĂŘWhy has the analysis performed only for May? It would be better to assess winter nutrient conditions (pre–bloom) and summer nutrient conditions separately.

Authors

We agree that it is more reasonable to analyze also pre bloom and summer conditions. In the next version we will provide this analysis (Fig. 4 of the current version) likewise for winter and summer.

Referee

– Nitrate should be included in the analysis.

Authors

We will include nitrate in this analysis.

Referee

Table3: – what exactly was assessed here? Dataset, time period, data handling. Taylor diagrams are a method to display the statistics presented not a method to derive statistics from. So, why not showing the diagram?

Authors

We think it will be better to provide the statistics in a more condensed way that allows to recognize differences in the performance of the individual parameters more easily. This is, from our point of view, done better with a table than providing an extra plot for every single parameter.

Referee

Stratification on the shelf and along the shelf break Fig 5 6 Fig5 cannot be used to explain Fig. 6. Fig. 5 is showing NS average values for T S, thus when averaging S over the NS you include the coastal areas and the Norwegian Trench region, which probably account for most of the S changes in the NS seas surface, but have no effect on the density profile at the shelf edge. Please include an analysis that supports your explanation. Obviously in the NS itself there is no change in winter MLD. Again please include references for parts of the discussion (e.g. on atmospheric transports). P16634L22 cycle, which P16634L27 PSU – please use SI units

Authors

Thank you for this comment. We already have chosen a more appropriate region on the continental slope excluding the Norwegian Trench which excellently supports our explanation. We will include all necessary references.

Referee

Decline in biological productivity P16636L16/17 this conclusion is only true for the considered scenario and model configuration.

Authors

We here refer to our reply to the major points above and to Fig. 1.

Referee

Potential Implications for industrial fisheries Here the authors include a rather speculative review paragraph concerning fisheries. The paragraph seems misplaced and not really related to the model simulation and scenarios. I would recommend excluding this paragraph and including a proper discussion instead.

Authors We will remove this paragraph in the next version.

Appendix P16643 L23 light absorbing particles P16644 to other studies

Figures: Fig. 1 NT lies on land Fig. 4 is very small, difficult to read Fig. 5 make labelling consistent Fig. 9 labels are difficult to read. Month abbr. check for German spelling (e.g. DEZ)

Authors

thank you for careful reading the manuscript.

Interactive comment on Biogeosciences Discuss., 9, 16625, 2012.

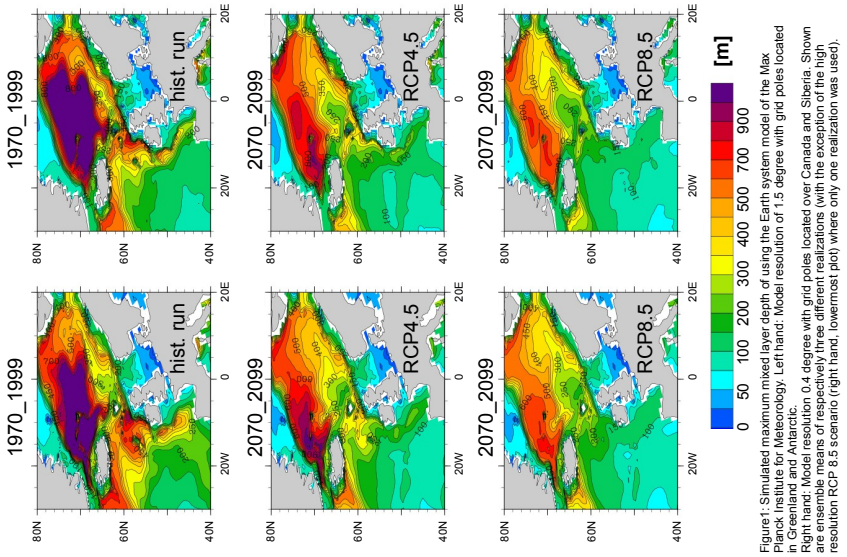

Figure 1: Simulated maximum mixed layer depth of using the Earth system model of the Max Planck Institute for Meteorology. Left hand: Model resolution of 1.5 degree with grid poles located in Greenland and Antarctic.
Right hand: Model resolution 0.4 degree with grid poles located over Canada and Siberia. Shown are ensemble means of respectively three different realizations (with the exception of the high resolution RCP 8.5 scenario (right hand, lowermost plot) where only one realization was used).

Fig. 1.